note on quantitative predictions

Our analysis is seriously independent of scale & notation used to express the result. Thus for a scale with radix r in which result is expressed with n_r digits we replace $\varepsilon^{n} \rightarrow \left(\frac{1}{n_r \Pi}\right)^{n_r} = \frac{1}{\Pi\, n_r^{n_r}}$

(a better perhaps $\frac{X}{n_r - 1} \cdot \frac{1}{r-1} \cdot \frac{1}{r^{n_r - 1}}$)

But best upper bound to number

$X = a_0 + a_1 r + \ldots a_n r^{n-1}$

expressible in form r^N is r^{n_r}.

So identifying approximate best upper bounds in this sense we have

$n_r\, r^{n_r} \simeq$ const independent of r.

Synthese Methodologies: Bayesian & Popperian.
Vol. 30 Nos 1/2 1975.

Very good article by Agazzi anti Bayes
Richard Jeffrey expounds Bayes.
P(H) may be very small - what we are alone interested in is $\frac{P(H/E)}{P(H)}$
not absolute value of P(H/E)

Giere cites Neyman-Pearson as equivalent to Popperian methodology.

Miller replies to Jeffrey Priors are meaningless subjective, if no practical use, nor can it be known, & if known, not objective.

Miller very interesting paper on the Accuracy of Predictions - shows how accuracy orderings can be "reversed" by redefining new parameters - argues against equivalent theories, the parameters are basic (cf Goodman's paradox)

Miller's reply to Jeffreys includes
this comment
"Popper's theory – that our decisions are
the more rational the more they are
subjected to severe criticism (in the
light of theories which are themselves
severely tested) – can do what Bayesianism
cannot do. It makes clear what
is rational about a rational decision".
Jeffreys dismisses this as mere waffle.
– But surely it is the Bayesians who
waffle.

Jeffreys criterion for degree of confirmation

$$\frac{p(H/E)}{p(H)} \text{ not good } = \frac{p(E/H)}{p(E)}$$

~~gun~~ confirms M and N if E confirms M
but E is irrelevant to N.

{ don't see relevance of
her example Newton + Kendal
E = orbits of planets.

He prefers $p(H/E) - p(H)$

He rejects $\frac{p(E/H)\,p(H)}{p(E)} = p(H/E)$

Synthese 1975

Jeffreys considers also likelihood ordering
which he rejects — (only other one ? plausible
[illegible] [illegible] $p(E/H) = 1.$?)

A. Musgrave?

Scientists work as hard cases they do <u>not</u> believe in { of Newton / Bohr }

(accept eff.ly by Howson)

Difference from Jon on old Ev.

Jon type of criterion is implausibility.

quite opposite to my view – Much – naked Bayesians

$P(C) = 0.95$

$P(R) = 0.5$

Hocking I. Phil Sci. 34 (1967) p 311 – 325.

"Slightly more realistic personal probability"

deals with dynamic assumptions

claim distinction between conditional prob.

$p(h/e)$ and probability per a post $p(h/e)$.

No total body against can justify

the dynamic assumption

"Conditional probability enables few comfort

a person knows all & judges that to cancel

out of the knowledge

but of the judgments & he can pass classify

to a new probability given that

process."

Hocking says probability dynamics is

a neglected subject – open to

Toffler to reveal Dean in

Hocking agrees with conditionalization,

suggested by P. Teller (Alliance Lewis and Putnam)

See Synthese 26, 218 (1973)

Paul Feller's views are supported by
William Harper in Synthese 30 (1975) 221-262.